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Discussions

Interactive comment on "The hierarchy of controls on snowmelt-runoff generation over seasonally-frozen hillslopes" *by* A. E. Coles et al.

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Author responses to Reviewer #2's interactive comments on: Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-564, 2016 "The hierarchy of controls on snowmelt-runoff generation over seasonally-frozen hillslopes" by A. E. Coles et al.

Authors: Summary

Thank you to the three reviewers (Reviewer #1, Reviewer #2, and S. Schälchli) for their in-depth reviews of this manuscript. Your reviews are very useful in improving this manuscript. We reply to each of the comments in separate documents. But we wanted to provide a brief summary here of what we saw to be the six main comments on the manuscript (some highlighted by more than one reviewer) that we will improve in the revised version:

1. Literature review. We will include more descriptions of what existing knowledge tells us about how each of the predictor variables individually influences runoff. We will extend our literature review to outside of the Canadian Prairies.

2. Transferability of findings. We will add more of a discussion on how these findings are transferrable to other regions.

3. Methodology. We will add more description of how the decision tree elements can be interpreted. We will make the methodology section less technical.

4. Predictor variable correlation. We will remove some of the predictor variables that correlate with others, and restructure the decision tree based on that.

5. Land cover and tillage. We will add tillage to the decision tree analysis, and add more discussion about land cover and tillage effects on runoff.

6. Implications for modelling. We will provide more discussion on specifically how this decision tree analysis can improve modelling approaches, such as statistical 'add-ons' to existing empirical approaches.

We talk about these six aspects in our responses, as well as several other comments that the reviewers had.

Below are the comments from Reviewer #2 and our responses to those comments.

Reviewer #2: The authors use a decision tree method and a 52-year data record of snowmelt related runoff from three agricultural plots in the Canadian prairies to study the controlling factors on runoff generation over seasonally frozen soils. Spring runoff conditions are characterized by determining runoff ratios. Decision trees are then used to understand the relative importance of a number of control parameters describing topography, land use, vegetation, and precipitation dynamics on the spring runoff. The intent of the study is to improve the understanding of the relative importance of the factors responsible for runoff over frozen grounds which should help to improve models describing infiltration into frozen ground and runoff over frozen hillslopes. The authors also want to give some advice to future measurement campaigns looking at seasonally frozen, snowmelt dominated regions. The authors claim that the major innovative part of their study as compared to other studies looking at runoff generation cover frozen ground is, that those studies usually are based on short term experiments and single season runoff events, while their study looks at averages over a much longer data period. This longer record should help to reveal the nonlinearities and interactions between the various process controls. In my opinion, this aspect of the study could indeed present some important new insights into the scientific field of snowmelt runoff generation over frozen ground. The paper is generally well written and the language and citing are appropriate. However, I find parts of it (especially in the Methods section) rather difficult to follow (see comments). The introduction and literature review focuses very narrowly on studies mostly from western Canada. While this is understandable due to the study location, I think a broader look at studies of runoff over frozen soils would be beneficial. The research goals on the other hand are clearly stated and well defined. The results and discussion section are adequate but could maybe be structured a little more clearly (see comments). The conclusions are based well on the results and provide a good summary of the study. The Tables are concise and easy to interpret, while some of the Figures could be improved (see comments) to make them easier to grasp. The topic of the article falls within the scope of the journal and does in my opinion represent a worthwhile contribution to the snow science community. However, the article needs in my opinion some fairly substantial changes to make it less difficult to follow. I would therefore recommend publication after major revisions (see general and specific comments).

General Comments

As mentioned, the literature review in the introduction should be broadened to include at least some studies of runoff and runoff generation over seasonally frozen or even permafrost soils. I would also welcome a discussion of the relevance of the studies result for other regions of the globe. Are the study results only applicable to prairie

landscapes or could similar hierarchies and interactions be found for example in high arctic locations that have frozen soils and often similar topography. This discussion could also be included towards the end of the paper in the general discussion section.

Authors: Thank you for your review of this manuscript. We appreciate your comments. We agree that we did not provide an adequately expansive review of literature on runoff over frozen ground, especially in regions outside of the Canadian Prairies. We will add a more comprehensive literature review in the revised manuscript. Yes – we believe the study results are applicable beyond the Canadian Prairies – to other gently sloping, frozen-ground environments of North America and northern Eurasia, but likely limited to similar agricultural, soil types. We will add a discussion section on transferability.

Reviewer #2: I found it somewhat hard to follow the explanation of the decision tree method in the Methods section. The description is very technical, which makes it hard for the average reader to understand what the analysis really means. I think it would be beneficial to include some explanations as to what it means f.e. if some predictors occur several times in a tree, what the ranking of the nodes mean for the importance to runoff generation, and what a leaf really represents “in reality”. I guess I would recommend to “dumb down” the explanation a little bit so that readers who are not familiar with the decision tree technique can appreciate what the single tree elements (nodes, leafs etc.) mean for the description of the “real life” process.

Authors: It is useful to hear this, thank you. We went back and forth in old versions of this manuscript with a technical vs. ‘dumbed down’ explanation of decision tree methodology and tree elements. We settled on a shorter, more-technical version following feedback from others that the methodology section was too long and we were over-explaining a ‘logical’ process. However, after hearing from you and another HESS reviewer that the methodology is hard to follow, we will revert to a more accessible and clearer explanation of decision trees. We will edit this in the revised manuscript.

Reviewer #2: I would really encourage a much more in depth presentation and discussion of the predictor variables as part of the discussion. The choice of which predictor variables to use is a major point of the study and should be treated as such. So far information on the predictors is only available within Table 2 and is very short. I would especially welcome a short description for each predictor, why they were chosen, and how they theoretically are expected to influence runoff. Additional points that could be discussed here are: What processes are believed to lead to the differences between total snowfall SWEf and snow cover SWEc. How closely are these two predictors related (maybe include an autocorrelation analysis). What impact does the soil fall water and fall soil profile water content have on the mechanics of spring infiltration (i.e. formation of concrete ice soils when water content is high, this might not be obvious to readers not that familiar with frozen soil hydrology)? Why is mean daily wind speed needed? The process deposition or removal of snow from the test plots due to blowing snow should be included in the difference between total snowfall SWEf and snow cover SWEc. So what additional information or impact does blowing snow during the winter have on spring runoff. The authors use 4 predictors to describe melt conditions. Wouldn't melt season length and melt rate be enough? What additional information do the other two predictors provide?

Authors: Absolutely. We will include further information on the predictor variable choices and how they theoretically, based on existing literature, should influence runoff.

The differences in total snowfall SWEf and snow cover SWEc are likely driven by over-winter redistribution by blowing snow and any over-winter melt events due to increased temperatures. As described in the comments above, because of the correlation between snowfall and snow cover, we will remove snowfall from the decision tree analysis in the revised manuscript.

Regarding a discussion of the impact of fall soil water content on the mechanics of spring infiltration, yes we will make this clearer in the revised manuscript and refer more to fundamental principles in frozen soil hydrology. We will include explanation of the effects of the soil moisture itself (like in unfrozen soils), but also temperature factors

such as heat transfer, melting and re-freezing, movement of water down temperature gradients, freezing of infiltrated water, and pore blockages by ice, etc.

We included mean daily wind speed above the blowing snow threshold as a predictor that might account for over-winter ablation of the snow cover (blowing snow and sublimation together can transport and sublimate up to 75% of annual snowfall from open, exposed fallow fields; Pomeroy et al., 2010). In other work by our group (Coles et al., 2017), we have shown that blowing snow is the main ablation mechanism when the hillslopes were in fallow, while over-winter temperature is the main ablation mechanism when the hillslopes had standing stubble. We will make reference to this effect in the discussion of the revised manuscript. Further, non-sublimated blowing snow forms drifts, which influence the streamflow peak and duration, which might in turn have knock-on effects on runoff ratio (Pomeroy et al., 2010).

Regarding the four predictors to describe melt conditions, we believed that they each provided useful information. Apart from melt rate and melt season length, we also included date of peak runoff and spring temperature gradient as predictor variables. Date of peak runoff is a useful measure as it provides information about the timing of spring runoff in the year: for example, whether it was early spring or late spring, which anecdotally has been thought to relate to the rapidity of melt, and infiltration and runoff amounts. We included spring temperature gradient as another indirect indicator of melt rate. You are right in suggesting that it was superfluous, and we can remove it from the analysis in the next manuscript revision.

Reviewer #2: Specific Comments

p.4 Line 5 and following The authors describe the usual conditions on the test plots as they were implemented in the decision tree analysis and the exceptions to those conditions. Unfortunately, considering the study period of 52 years there seem to be quite a lot of extraordinary conditions considering land use and tillage. One would assume that especially different tillage practices and the presence of stubble or standing crop over the winter (additional catch of blowing snow as mentioned later in the paper) could have quite an impact on runoff. Yet it seems like all years were included in the analysis. At the very least I would expect a discussion of the possible impacts of this.

Authors: Actually, this comment isn't entirely accurate. We didn't only implement the usual plot conditions into the decision tree analysis and ignore the exceptions. We included all conditions in the analysis: the decision tree analysis directly includes the land covers for each plot and each year (whether 'usual' or 'exceptions'). Further, we don't agree that the exceptions amount to "quite a lot"; the exceptions to the usual wheat-fallow land cover rotation amount to only 16% of the occurrences, and the exceptions to the usual conventional tillage practice amount to only 11.5% of the occurrences. But again, we did differentiate between these in the case of land cover. We did not include tillage differences in the decision tree analysis. This was partly because previous work (Coles et al., 2017) had shown that long-term runoff trends are similar across all three hillslopes (no difference in the magnitude or significance of trends, despite the fact that the tillage practice on one of the hillslopes - Hillslope 2 – had been changed from conventional to zero-till in the last 18 years of the record), which might suggest that tillage changes have not played a role in the runoff regime. We will refer to this study in the revised manuscript. However, theoretically, tillage would affect runoff ratios due to influence on soil structure, macropore development, surface water retention and infiltration. We can therefore explicitly include this variable in the revised manuscript.

Reviewer #2: p.6 line 25 The runoff ratio is defined as total runoff divided by SWEf from the end of the hydrological year to the end of snowmelt. How was liquid precipitation (i.e. rain) handled in this? I'm aware that in Saskatchewan rain after the end of the hydrological year and before the start of the snowfall and during snowmelt is rather rare, but it must have occurred sometimes during the 52 year study period. Also how was SWEf measured at the met station (precipitation sampler, snow depth on ground)?

Authors: Rainfall during this time period was indeed rare, yes. We used an all-weather Belfort weighing gauge (information given on p.5 line 11) and distinguished between

snowfall and rainfall using the air temperature data (we will add this method of distinguishing between snowfall and rainfall to that section). We summed all snowfall instances to give total seasonal snowfall, SWEf (mm).

Reviewer #2: p.11 Section 4.2 This is a very minor point. Here the authors present the results from the secondary decision trees that split the dataset into high or low expressions of the six key variables. The results are presented in Table 5. The results start with a discussion of high and low snow cover years and then moves on to high and low total snowfall. These two variables are presented in reverse order in Table 5. You might want to change either the Table or the results section.

Authors: Good point, we will reverse the order in Table 5.

Reviewer #2: p.12. line 5 The authors describe that the top three controls on runoff ratio matched the overall hierarchy for high snow cover years, albeit with differing orders of importance. Maybe you could quickly include that changed order so that the reader does not have to look for Table 5 to find out what that order was.

Authors: Good point, we will add this in for easy-reading.

Reviewer #2: p.12 line 11 The authors state that for low fall soil surface water content the analysis indicated that mean daily wind speed had a large influence on runoff ratios. Could you maybe discuss why this could be the case. The connection is not clear to me at all.

Authors: Blowing snow creates drifts, which, on these Swift Current hillslopes, are in the immediate vicinity of the runoff flumes. This might affect runoff ratio due to the drifts' influence on streamflow peak and melt duration. There might also be a statistical reason for the selection of mean wind speed in the decision tree model. We will discuss this further in the revised manuscript.

Reviewer #2: p.16 Figure 4 This Figure is very difficult to understand. It needs to be explained in much more detail. Also the inserted panel in the right hand figure showing

Figure 3.3. (why 3.3?) is way too small to be readable. I would recommend removing it entirely. The left hand panel of the Figure is not explained in the text at all and the rudimentary legend box is not enough to make it understandable.

Authors: Thanks for this feedback on this figure. The inserted panel is a small version of Figure 3 (apologies for it saying “Figure 3.3”, this was an error) and is not intended to be readable, but solely to remind the reader that Figure 4 and its colour-coding are dictated by the findings and colour-coding of Figure 3. However, we appreciate that it might be confusing to have this insert here at all, and will trust that simply stating in Figure 4’s caption that it is “colour-coded using leaf colours in Figure 3” is enough and we therefore do not need the insert. We will remove the insert. Figure 4 is an attempt to return to the scattered relationship between precipitation and runoff (Figure 2) and account for this scatter using the findings of the decision tree (Figure 3; Tables 4-6). We state this in Figure 4’s caption and in the main text at p.16 line 9-10. However, we will add further text to make this clearer. We will also improve the legend (which is showing the principal conditions that drive the runoff response in each part of the partitioned scattered figure) to make that clearer.

Reviewer #2: P17. Line 29 The authors state once again that the six key variables explain most of the variability and exert the greatest control on runoff ratio. Maybe one could add a discussion here of whether including only these six variables is “good enough” for a model or how much additional information the other 9 used parameters really provide.

Authors: We stated in the manuscript that those six variables explain 70% of the runoff ratio variance. Whether that is ‘good enough’ for modelling is likely subjective and depends on the desired performance of the model. Compared to the 14.3% of the runoff ratio variance that is explained by an existing empirical modelling approach (discussed in section 5.3), this 70% is a vast improvement. We think that including 6 parameters in a model that has 140 observations is already a lot, and including any or all of the remaining 9 would risk over-parameterisation. We will add a small discussion in this

section about this. Reviewer #1 wondered about what realistic modeling improvements we could offer from the findings of this study. We suggested a statistical, error-term type add-on to existing empirical two-parameter (soil moisture and SWE) approaches that would account for the variability described by the additional four parameters found here.

Reviewer #2: P18 Figure 6 Intuitively I would have expected the wet and dry scenarios in panel D to be presented in different order, i.e. wet on top and dry on the bottom.

Authors: Yes, that would be better. That way all ‘high runoff ratio’ examples are on the top row, and ‘low runoff ratio’ examples are on the bottom row. We will change this.

Reviewer #2: p.18 end This sentence is virtually identical toa sentence on p. 16 lines 12 and following

Authors: Yes, it is the same point. However, that was intentional. The paragraph on p.16 lines 5-13 is the start of the discussion section, and is a summary of the discussion that is to follow in the next sub-sections (highlighting the main results that will be discussed). We would prefer to keep it like this.

Reviewer #2: p. 20 line 14, 15 If the relationship was so strong, why wasn’t length of frozen soil over the winter not included as a predictor? Please add explanation.

Authors: This was later analysis that was carried out after the decision tree analysis in a direct attempt to explain why sometimes soil moisture memory appears to be strong and other times not. It was analysis based on a learning outcome and discussion of the decision tree findings, and was not considered prior to the decision tree analysis. As mentioned above in response to a comment by Reviewer #1, we will include a measure of frozen ground depth at the time of runoff, into the decision tree analysis.

References

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Pomeroy, J., Fang, X., Westbrook, C., Minke, A., Guo, X., & Brown, T. (2010) Prairie Hydrological Model Study Final Report. Saskatoon: University of Saskatchewan.

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